

Rejoinder to BOLLINO's & SALA's considerations about proposed synonymy of some *Papilio alexanor* subspecies

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Having received the critical manuscript of BOLLINO & SALA (1995, this issue) on our paper on *Papilio alexanor* (DAVID & SANETRA 1994) from the editor of the journal "Nachrichten des Entomologischen Vereins Apollo", we feel forced to write this short reply in order to make some aspects clear to the reader, especially because the authors refer to an unpublished letter not known to the reader.

Undoubtedly, the category of subspecies remains one of the most troublesome subjects in butterfly taxonomy with a number of contradictory concepts (e.g., EITSCHBERGER [1984], KUDRNA & GEIGER 1985, HOFMANN 1993). BOLLINO & SALA (1995) characterized our subspecies concept as being restrictive, although we mainly agree with the widely accepted biospecies concept of MAYR (1969). Accordingly, populations referable to distinct subspecies inhabit a geographic subdivision of the species' range and are different from each other in taxonomic characters. At the same time subspecies are dependent on biological information, which is very rarely available, and only exceptionally proved in nature (KUDRNA 1986). However, in contrast to the conclusions of KUDRNA (1986), we do not wish to reject the application of the term subspecies in lepidopterological taxonomy, but we agree with him that various morphological and biological principles must be fulfilled to facilitate an efficient assignment of subspecies names. We therefore demanded the inclusion of additional data into subspecies descriptions, like foodplant records, life-habits and in particular the plausibility of reduced interbreeding. The mere documentation of a "local constant variation" within a species sensu BOLLINO & SALA (1995) might be enough to characterize a local population group, for which, in our opinion, it is very exaggerated to assign a scientific name of its own. Moreover, such classifications are useless from the practical point of view (KUDRNA 1986) and seriously reduce the scientific value of subspecies names. Rather, the term subspecies can more reasonably be applied to infraspecific units with eco-ethological differences, which we consider to be of greater evolutionary importance than slight morphological

ones. That does not mean that we totally neglect morphology, but we wish to emphasize that adjacent subspecies should generally feature the property of reduced interbreeding. For instance, the stability of their ranges may be explained by selection against hybrids. A classical example for valid subspecies in this sense, which we have already cited in our paper (DAVID & SANETRA 1994: 20), are the two parapatric subspecies of *Papilio glaucus* in North America (HAGEN 1990).

In fact, both descriptions of *Papilio alexanor* subspecies recently published by our Italian colleagues (SALA & BOLLINO 1991, BOLLINO & SALA 1992) mostly deal with little morphological differences of the new subspecies without providing any detailed information concerning their biological properties as required by the above given definitions. Wing markings are the only characters on which conclusions were built, and even standard procedures like comparison of genital morphology were disregarded. This kind of treatment appears to us a much more limited concept of subspecies than ours, and we consider such descriptions too scarce for establishing one subspecies after the other. Therefore we do not accept the view that subsequent authors shall give much more precise information to propose synonymy of carelessly erected taxa. We believe that authors in their first description must bring enough evidence for the justification of their new taxa. If not, synonymy may later be established just by demonstrating the insufficiency of the original description. Our taxonomical considerations on *P. alexanor judaeus* have indeed in part been aimed to stop "subspecies superfluity" and to remember lepidopterologists to their scientific responsibility. Even though BOLLINO & SALA (1995) state that most entomologists would agree with their subspecies concept, in comparison with other insect orders, it must be noticed that such a concept is forwarded by some lepidopterologists only.

It has never been the primary goal of our study to analyze foodplant records in the Eastern range of *P. alexanor* in detail, as was clearly expressed in the title of our paper. However, we wanted to give a general information on what sort of plant caterpillars in this area usually feed on. Because we had been totally aware of the fact that most records from literature would not prove reliable, we chose the term "*Ferula*-Arten" for our generalized comparison of foodplants. BOLLINO & SALA (1995) now provide a plenty of new and hitherto unpublished data which are very interesting, but not really relevant in this discussion. The more it is noteworthy that the same authors three years ago in the description of ssp. *eitschbergeri* have also believed "*Ferula communis* to be the original foodplant" (BOLLINO & SALA 1992: 130), and that we did not had access to

their unpublished data cannot be regarded as a shortcoming. Nevertheless, the new data make clear that *P. alexanor* is oligophagous in its eastern range and much more investigations are necessary to show if some populations differ markedly from others in their life history. Presently there seems little reason to suspect this as all putative subspecies feed on closely related plant genera with overlapping patterns (*atticus*: *Opopanax*, *Pimpinella*, *Ferulago*; *eitschbergeri*: *Opopanax*, *Ferula*; *judaicus*: *Ferula*, *Heptaptera*). In contrast, a rather different strategy occurs in *P. a. alexanor* whose caterpillars have adapted to a completely different umbellifere, *P. saxifraga*, and as we have recently shown are monophagous within the south-western alps except for some populations from the Mediterranean coast. Furthermore, we have predicted in our work (DAVID & SANETRA 1994: 18) that areas might exist in which both *Opopanax* and *Ptychotis* are used as foodplants, possibly defining a hybrid zone between two subspecies. Again BOLLINO & SALA (1995) reproached us for not knowing unpublished information upon the existence of such a location in the maritime alps. However, host use in this area has to be studied in greater detail as was proposed earlier (DAVID & SANETRA 1994: 20).

We want to make absolutely clear that we will uphold our point of view that *P. a. alexanor* caterpillars are monophagous on *P. saxifraga* (with the given exceptions, see DAVID & SANETRA 1994) unless real evidence is given for the regular use of food sources other than *Ptychotis*. The argumentation of our colleagues with respect to their record of *Trinia glauca* as a foodplant in Valdieri appears doubtful, since different versions for explanation has been provided. Whereas in the original description (SALA & BOLLINO 1991: 78) exclusively one foodplant, *Trinia glauca*, was reported, G. SALA in his letter of August 1994 to one of us confirmed *P. saxifraga* as a foodplant in Valdieri, but some caterpillars were found on *Trinia glauca*. From their latest version (BOLLINO & SALA 1995) it can be deduced that they have never identified *P. saxifraga* as a foodplant in Valdieri as yet. The authors assumed our discordance to come from the different observation times and referred to different flowering of *Trinia* and *Ptychotis*. Indeed we visited Valdieri on 9 July 1992 ("1993" in DAVID & SANETRA 1994 is due to a printing error), in which year it had been a very cold spring, and considering different climatic development it seems unbelievable that the two weeks difference between our observations in early July and the capture of the holotype on 24. VI. 1989 should have been of great relevance. It were SALA & BOLLINO (1991: 78) themselves stating that "the flight period is very constant and covers the second half of June and the first half of July". Thus, different foodplants with

echeloned blooming are very unlikely in this case, as there is no indication for the postulated prolonged emergence period. The given flight period at Valdieri is absolutely consistent with our observations on French populations, strongly suggesting *P. saxifraga* to be the only regular foodplant there as well. Females lay their eggs on flowers which are just beginning to open, as usually is the case in *Ptychotis* at the end of June. *Trinia glauca*, if present in Valdieri, has almost reached the end of its flowering period when the first *alexanor*-females appear. Overall, life history of the putative ssp. *radighierii* appears to be the same as in adjacent French populations and reduced interbreeding with nominotypical populations is highly improbable. The reader may now be able to decide himself whether he wishes to call such things subspecies any longer.

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